

Parvin, (J.)

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ANNUAL ADDRESS

OF THE

PRESIDENT OF THE PHILADELPHIA  
OBSTETRICAL SOCIETY.

BY

PROF. THEOPHILUS PARVIN, M.D.



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## PRESIDENT OF THE PHILADELPHIA OBSTETRICAL SOCIETY.<sup>1</sup>

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GENTLEMEN, — Few honors are possible to the American obstetrician which equal, fewer still which exceed, the Presidency of the Philadelphia Obstetrical Society. The very name "Philadelphia" is one of power and praise in the history of obstetrics, for in this city rather than in any other of our country there lived those who were most eminent as writers and teachers in this department of medicine, and who were chiefly quoted two or three generations back as its chief American representatives. Some illustrious members of the profession, a few departed, others abiding, who, by useful and honorable work, advanced obstetric science and art, have occupied this chair. Some of the monographs presented to this Society contain treasures of research or observation which every student consults, and are professional classics. Reports of cases here made, and discussions here had, have, by the ever multiplying and everywhere scattered pages of the medical press, become the common property, the valuable and valued possession, of educated doctors the world over.

Thus recognizing the honor done me, my tongue would not be true to my heart did it not utter words of thanks to those conferring it, though

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<sup>1</sup> Delivered at the meeting on March 7, 1889.



words can but feebly echo the deep gratitude felt. Let me now also record my hearty appreciation of the magnanimous course of him who moved, and of those who supported the motion, that the choice be made unanimous. Heaven graciously grant that none ever repent this action. Pericles, when dying, said the greatest and most excellent thing of his life was, that no Athenian ever through his means wore mourning. In like manner may it be my happiness to say, upon retiring from this chair, that no member of the Society ever had just cause to mourn any injustice or discourtesy from me.

Having been a medical teacher for about a quarter of a century, and during much of this time actively engaged as a medical journalist, permit me, in a part of the address which custom demands for the occasion, to say a few words chiefly suggested by this experience.

One who constantly refers to, or even occasionally consults, medical periodicals, is not unseldom annoyed or sorely hindered by the failure of writers to give clear and appropriate titles to their work. Though attention was vigorously called some years ago by our countryman most familiar with medical literature — Dr. J. S. Billings — to this matter, the desired and needed reform has not been made. Medical societies still occasionally hear, and medical journals still report, “singular,” “remarkable,” “unusual,” “extraordinary,” “wonderful,” “unique,” et cetera, “cases.” A man would hardly call the child sprung from his loins “Nothing” or “Anything,” “Nobody” or “Anybody,” but he sends out to the professional world the Minerva child of his brain bearing an utterly meaningless name. The professional knowledge offered us ought to be properly labelled quite as much as drugs exposed for sale, and correctly named, just as is the food on a bill of fare.

Another evil in this literature is haste in the reporting of cases. Not only in our obstetrical society, but quite as much in other societies, there are occasional instances of *partus prematurus* and of *partus immaturus*, but even of abortion. If I do not forget my reading, in Horace’s “Art of Poetry” the poet is advised to keep his production nine years before making it public. Peradventure a pregnancy of nine months would be, in some instances, advisable in order to secure the viability of certain cases, for in that time their history would be completed, and the lessons they taught be clear. When one walks upon the sea-shore in the early dawn he finds here and there jelly-like masses which the receding waves have left; but after the sun has been up a few hours, and its fierce rays have beaten upon those masses, they shrink, they shrivel, they vanish, scarcely leaving a trace upon the white, dry sand. And thus it may happen in like manner with cases taken untimely from the womb; they

become lifeless and little when the clear, warm light of truth and experience shines upon them. A true fact is most valuable, but a half-fact is of trivial value in the temple of medicine; we all need, too, to remember the words of Cullen, that false facts are more numerous than false theories.

Of the very few in the medical profession who knowingly falsify facts, who bring forward untrue statistics, striving to exalt themselves upon a rotten pedestal, claiming a vast number of cases which thoughtful men will not believe they ever had, it may be said that they have had their day; those who do not know them, or who do not investigate the probabilities of truth, accept their facts, and are misled; but there is always a saving remnant among doctors, as well as in society, who are not betrayed into a false faith.

No error of reasoning is more common among men, medical writers included, than drawing a conclusion from incomplete or insufficient data. A doctor, for example, has two or three cases in which such a pathological condition existed, or was believed to exist, and the cure was effected by certain means; therefore in all cases of the disorder that condition must be present, and all can be cured by the same therapeutic means. Hence the most positive opinions and downright dogmatisms are given the profession as knowledge. And indeed there are those who find their best and most acceptable diet in such peptonized food. It saves them all intellectual digestion, — that is, the trouble of thinking; and they become so fascinated with positive and dogmatic teaching, that they even cry out against those who are not dogmatic. It might be a good thing if in every medical college medical logic were a part of the regular course of instruction.

An evil sometimes done the profession is the constant publication of successful cases and the omission of failures.

“How I found Livingstone” is intensely interesting, but I have sometimes wondered whether a volume entitled “How I did not find Livingstone” would not be equally interesting, and even more useful to the African explorer. Macaulay probably deserves to be ranked with Pythagoras, Samuel Johnson, and Coleridge, as among the most marvellous and instructive talkers the world has ever known; and yet it has been said of his so-called conversation, more monologue than conjoint speech, that it would have been more interesting if it had been interrupted by an occasional brilliant flash of silence. The report of a series of successes would have greater value if interrupted by an occasional failure; to the individual, a failure is often more valuable than a success, and the instructive lesson ought not to be denied the profession.



The last remark leads me to a few observations upon the objects had in view in medical writing, — the motives determining authorship. Very many write because they are paid for their work, and often this work is entirely congenial. Frequently their names are not attached to these contributions, so that no additional compensation comes through reputation acquired by writing. Certainly some of the best of the writing done in our medical periodicals is solely inspired by pecuniary reward, just as some valuable work in general literature has been the product of the same motive; Walter Scott's pecuniary liabilities gave us the *Waverley* novels, and Johnson wrote "*Rasselas*" to pay the funeral expenses of his mother.

Some men write from an honest love of fame, or this great gift denied, — since it is granted to only few among the few, — at least they will strive for temporary notoriety. Let no one despise "that last infirmity of noble mind" as a motive, or seek to repress the honest efforts made from its influence. But he who feels this inspiration must not only be content

"To scorn delights and live laborious days,"

but also be sure he finds something valuable to say to the profession, and says it in the best possible manner; that is, in the fewest and plainest words, for Pharisees only thought they would be heard for their much speaking. The ground that occasionally lies fallow gives richer harvests than if its fertility is exhausted by constant production.

In "*Haifa, or Life in Modern Palestine*," the author, Laurence Oliphant, speaking of that man, among the bravest and noblest of the century, or of all centuries, — General Gordon, — remarks, "His modesty was such that I could only compel him to narrate his own adventures by a process of severe cross-examination." Some doctors are equally reticent as to the clever things they have done, and it is only by searching inquiry that you can secure from them facts of value to the profession; while occasionally you meet with one who does not wait any cross-examination, severe or mild, but is perpetually telling of his "last case," until, in your weariness, you almost wish it were his "last" one.

No man ever yet, even though he had the strength of Hercules, lifted himself "to dwell in the blue serene of fame" by tugging at his boot-straps; a man by self-inflation may increase his superficial dimensions, but not his intrinsic weight. Or, making more delicate comparison, and borrowing from Coleridge, "He that will fly without wings must fly in his dreams; and, till he awakes, he will not find out that to fly in a dream is but to dream of flying."

Some doctors write, hoping, it may be, that while their words will be useful to the profession, hoping still more that they will bring them patients, they cast bread upon the waters, expecting to find it before many days. Writing for practice is certainly very often done, and by some ably done; the motive of this work may be patent, or latent, but the writer often secures his reward. The special temptation to one who thus writes is to unduly exalt himself, and ignore or depreciate other workers equally able and distinguished; a capital<sup>1</sup> I is almost as frequent upon a page as the many quills of the fretful porcupine; such work is liable to be imperfect, incomplete, and lacking in catholic character, and it occupies possibly an honorable place, but it does not abide among the best literary treasures of the profession.

One cannot readily suppose that Hippocrates, Boerhaave, Hufeland, Harvey, Watson, or, coming to our own city, Dewees, Hodge, Wood, Meigs, or Parry,

"Tongues of our dead not lost,  
But speaking from death's frost  
Like tongues of fire at Pentecost,"

wrote for the purpose of securing clients. Listen, I pray you, for a moment to the words of one of the noblest of English physicians, Sydenham, and see if they do not waken prompt response in your hearts, so that "ancient founts of inspiration well through all" our "being yet."

In writing to a friend who has commended one of his "Treatises," this exemplary man said: "And indeed I have so seldom received anything of this nature, that either I have merited no such thing, or else these candid and ingenuous men, whom nature has framed with such excellency of mind as to know how to be grateful, are very few, — scarce so many as the gates of Thebes or the entries of wealthy Nile. Yet, notwithstanding, I endeavor all I can, and will do so, to learn and promote the method of curing diseases, and to instruct those that are less conversant in practice than myself — if any such there are — let other people think of me as they please. For, having nicely weighed whether it is better to be beneficial to men or to be praised by them, I find the first preponderates, and much conduces to the tranquillity of the mind. But as for fame and popular applause, they are lighter than a feather or a bubble, and more vain than the shadow of a dream. But if any one thinks

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<sup>1</sup> There appeared in one of our medical journals, not long since, a contribution of thirteen sentences, and "I" occurred ten times, though there was not an original thought or suggestion in the article. If men must be vain, would it not be well were their egotism at least occasionally hidden behind the decent drapery of altruistic speech?



that riches got by such a reputation have something more of solidity in them, let them enjoy what they have scraped together with all my heart; but let them remember that some mechanics of the most sordid trades get and leave more to their children; yet they do not exceed the beasts in this, who take as much care as they can of them and theirs; and if we except things honest proceeding from the judgment of the mind and virtue, whereof the nature of beasts is not capable, the beasts are plainly as good as these and all others who do not endeavor to do all the good they can in their generation."

Thus the endeavor to do all the good we can in our generation is the high aim of the best professional work.

The great prominence given in this Society for some time past to surgical gynæcology, to the relative neglect of medical gynæcology and obstetrics, has been matter of public and private criticism, if not sometimes even of complaint. I confess to having fully sympathized in these opinions. And yet careful reflection has led me to doubt the justice of any explicit or implied censure, for a retrospect of the marvellous progress in abdominal surgery during those recent years, especially as it relates to pelvic inflammation in the female, has compelled the conclusion that these things must needs have been as they were. Let us go back forty-one years to the beginning of the work of Bernutz, who was the herald of our clearer day. In 1848 this laborious investigator published a series of papers upon<sup>1</sup> "The Accidents caused by Retention of the Menstrual Flux;" in 1861 and 1862, these papers, with others dealing especially with inflammation of the pelvic peritonæum, were issued in two<sup>2</sup> volumes, which included more than thirteen hundred pages. In 1867 a much abridged translation of the work was published by the Sydenham Society. Now the great value of Bernutz's many years' laborious investigations was, not merely or mainly that he swept away the hitherto generally received pathology of the most frequent form of pelvic inflammation in woman, proving that in the vast majority of cases this inflammation involved the serous, and not the connective, tissue, but that this peritoneal disease usually originated from disease of the uterine appendages.

As I re-read in the preface to these volumes the words of Bernutz, stating that he had laid aside every other sentiment save the desire to be useful to the sick, and that this was the sole motive for writing the book, I do not wonder at his having accomplished so much in opening the way for one of the most important advances in surgical gynæcology; for the

<sup>1</sup> Arch. gen.

<sup>2</sup> Clin. med., sur les maladies des femmes.



best success is the reward of the highest motive. Again: was there not something almost prophetic in the second of the following sentences also found in this preface? "It may be understood from what precedes, that, notwithstanding the labor in the preparation of this work, we acknowledge its many deficiencies and also many imperfections. We have even thought it might be of only a transitory character, — a point of departure for other works."

The affection of the uterine appendages, which Bernutz sometimes called the orchitis of women, possibly thus pushing an analogy too far, had in nearly one-half of the cases a puerperal origin; that is, occurred after labor or miscarriage. Then in more than one-half of the remaining cases the disease was gonorrhœal; and, to add to the force of the last statement, and to show that operators who so frequently find pus collections in the tubes have a probability in favor of their view, let us remember that nearly one-third of the women suffering from gonorrhœa, under Bernutz's observation, had inflammation of the pelvic peritonæum.

More than twenty years ago I reviewed this work in the "American Journal of the Medical Sciences," and one of the pleasant reminiscences, now saddened by the death of Bernutz, was a letter from him thanking me for the review.

How those of us, years ago casting aside the cellulitis which occupied so important a place in text-books and in clinical teaching, accepted the new doctrine, but sadly found that change of name did not add to our therapeutic resources! We tried to work out at the bedside the problem of treatment, the method of cure, and our experience often reminded us of the wise master's words when he said, "We have described the recrudescences which occur in *pelvi-peritonites*, because these recrudescences are so frequent that they constitute an almost essential character of the affection." We plied our leeches, our opium, and our blisters, as taught by Bernutz; the majority of patients recovered, some, though, became chronic invalids, and others died. What new light could come? It was reserved for Lawson Tait, one who is an image-breaker rather than an image-worshipper, possessing a mind remarkably practical, and a delicacy and intelligence of touch that are marvellous, standing to-day the foremost of abdominal surgeons, to put in practice a creed that might be thus formulated: If the uterine appendages are so diseased that cure is impossible, and their condition is the cause of recurrent attacks of pelvic inflammation, these recrudescences upon which Bernutz so earnestly dwelt, — inflammation which produces great suffering, hinders useful living, and may end fatally, — remove those appendages. In 1873, Mr. Tait first performed the operation to which his name has been given by

some, but which is more appropriately known as the removal of the uterine appendages. Less than sixteen years have passed, and the operation, while slowly meeting professional recognition, has not yet been fully appreciated in regard to its imperative necessity in certain cases. Many a woman drags out a wretched existence or prematurely perishes because the recognition of diseased uterine appendages is not made, and of course they are not removed.

It is so easy on a *a priori* grounds to criticise or to condemn this operation. It may be called mutilation, sterilizing, unsexing. One might repeat the words of Plutarch, "For to use the knife, unless in the extremest necessity, is neither good surgery nor wise policy, but in both cases mere unfeelingness; and, in the latter, unjust as well as unfeeling." But facts are stubborn, and if they are many enough and strong enough will vanquish *a priori* reasoning and prejudice. These facts needed to be accumulated in order that the profession should know that Mr. Tait and his followers were right; there was a necessity for what poor Hood called the piscatorial precept, "line upon line." While some operators in this city — I wish I could mention all their names in the order of their work — preceded Dr. Joseph Price, by far the greater number of the so-called Tait operations have been done by him, and by those whom he assisted or instructed. The slight mortality his statistics show, and the happy results in those recovering, present an unanswerable argument. So, apparently, there was need that blind eyes should be brought to see and deaf ears to hear, and thus case after case presented to the Society, and to the far larger number of physicians who know the organization only through its published transactions.

Something, too, must be allowed for the zeal of discipleship. Something, too, for the fascination which a new and important therapeutic means excites. Guizot once said that when a man is profoundly impressed with an idea, he becomes a missionary of that idea.

Thus concluding this topic, I cannot think that too much importance has been given in the Society to gynæcological surgery. But is the future to be but a reflex of the past? Has not the time come when obstetric subjects should be given greater prominence? At least partial answer shall be made in the next and final topic presented in this address.

The story is told that once a man, when asked to give a definition of walking, made no reply in words, but rose from his seat and walked. Permit me, therefore, without argument for the importance of having obstetrical papers more frequently presented to the Society, now to present such a paper as an appendix or addition to this address.



## ANNUAL ADDRESS.

### REPORT OF A FATAL CASE OF PUERPERAL ECLAMPSIA.

Let me premise that I am indebted to Dr. J. L. Rothrock, the resident physician in charge of the patient, for the report of the case,— a report which will be presented in a condensed form, — and also to him for some statistics of eclampsia occurring in the Philadelphia Hospital, and to Dr. Eshner for the record of the autopsy, which was made under the direction of Dr. William Osler.

O. K., unmarried, primipara, 17 years of age, was received in the maternity on the 5th of January, 1889. At 9 P.M., soon after her admission, had a convulsion. An examination of the urine showed that it was highly albuminous, and contained some granular casts. By vaginal examination, the os was found sufficiently dilated to admit two fingers, and the pelvis roomy; the presenting part was believed to be the vertex, but the head was so high that it was impossible to make a positive diagnosis. Coma followed the convulsion, and lasted nearly half an hour, when partial recovery of intelligence occurred. At the end of the half-hour a second convulsion occurred, and a third fifty minutes after the second. She was then given thirty grains of chloral by the rectum, and one-fourth of a grain of morphia hypodermically. Three more convulsions occurred before 2 A.M., when Dr. Parvin saw her; she had previously a second hypodermic of morphia. Forty grains of chloral were given by the rectum, and chloroform by inhalation. The os was found as large as a silver dollar, the uterine contractions regular, and artificial dilatation of the os was employed with a very satisfactory result, complete expansion being accomplished in half an hour. No convulsion occurred during the dilatation. The membranes were ruptured, and the head descended into the pelvic cavity. At 3 A.M., the seventh convulsion occurred, but it was less severe than some that preceded it, and the labor ended spontaneously at 6 A.M., the child born partially asphyxiated, and with difficulty resuscitated. In twenty minutes after the birth of the child the eighth convulsion occurred, and during it the placenta was expelled. After this the patient had a tranquil sleep, and seemed better. But in little more than an hour there was another convulsion, and this was followed by a second and a third, the intervals being only fifteen minutes, and one of the attacks lasting five minutes. In three hours another convulsion, and still another nearly three hours after that, and it was the last. There were thus in all thirteen distinct convulsive seizures, while several apparently were averted by the timely use of chloroform. The patient died on the morning of the 7th, about thirty-five hours after the first convulsion. The temperature was taken repeatedly, and was never high, only once reach-

ing  $102^{\circ}$ ; it was generally found less than  $101^{\circ}$ . An hour after death the temperature was  $98.8^{\circ}$  F.

In regard to the obstetric management of the case, it may be asked if the forceps ought not to have been used as soon as the os was well dilated. The labor pains were active and efficient; it seemed to me that it would be a needless violence to drag the child's head rapidly through an unprepared birth-canal in a primigravida; and only one convulsion occurred during the second stage of labor. Moreover, confiding too much in the testimony of the thermometer, I was quite hopeful as to the patient's recovery. How satisfactory it would be if, by frequent thermometric observations in eclampsia, we could know the patient's true condition, when peril comes, and when it disappears, just as the sailor safely guides his vessel over a bar by frequent soundings. Bourneville asserted that if eclampsia was to prove fatal, the temperature was progressively elevated, reaching a high degree; but the record in this case proves that the assertion was too absolute. So, too, the observation of the temperature, in this case taken after death, does not confirm the statement of Hypolitte as to its great increase.

It is probable that, as Schröder claims to have first pointed out, the condition of the pulse in the eclamptic is of the greatest significance in forming a prognosis. My experience in this case will lead me to attach less importance to the temperature than I have heretofore.

Body of a well-built, well-developed young girl. No œdema. Face suffused. Skin of scalp much infiltrated with blood. Skull-cap thin; diploe extremely vascular. Dura dark red in color; pial veins showed through it with great distinctness. Large clot in longitudinal sinus. Vessels of pia, large and small, deeply congested, down to the smallest venules. On section, gray matter unusually red in color; white matter highly vascular; venous points numerous and large. No special regions of anæmia. The congestion seemed quite uniform. The basal ganglia were normal. Ventricles showed no change. Ependymal veins unusually prominent. Structures at base normal. On careful examination of all parts nothing was discovered but intense congestion.

*Thorax.* — Extensive adhesions on right side. Pericardium smooth; cavity contained three or four ounces of fluid. Right and left auricle contained dark, grumous blood; ventricles empty and contracted. Weight of organ, after removal of blood, thirteen ounces. Valves normal. Walls of left ventricle distinctly hypertrophied, measuring in places three-quarters of an inch; the chamber itself not enlarged. On microscopic examination the muscular tissue showed marked fatty degeneration. Aorta normal.

*Lungs.* — Right lung crepitant throughout; contained a good deal of blood and serum in dependent portions. No consolidation. Left lung: apex and superior two-thirds presented extensive pneumonic consolidation. At upper and anterior



part of lobe, the consolidation was uniform, reddish in color, and looked like ordinary pneumonia, but in the remaining portion of the lung the affection was limited to small areas of grayish-white appearance, surrounded by areas of deep congestion. The lower lobe of this lung was unaffected. The condition was a typical one of catarrhal pneumonia. At the lower part of the lobe were one or two spots which looked quite softened and were suspiciously like puriform softening. The pleura covering the inflamed region had a thin sheeting of fibrin. The bronchial glands were a little enlarged.

*Abdomen.* — Spleen not enlarged.

Stomach and intestines showed no change.

The kidneys weighed together ten ounces. The capsules detached readily, leaving a mottled surface, with the stellate veins unusually distinct. On section, there was marked contrast between the cortex and the pyramids. The former was pale and the medullary rays looked swollen. The vascular lines, in places, did not show. There were no signs of fatty degeneration. The organ appeared to be in a state of acute nephritis, passing into the second stage.

A. A. ESHNER.

The only remark upon the autopsy now made is that it furnished no support to the theory of eclampsia in its essential character, which the late Angus MacDonald presented in 1878. After describing the post-mortem results in a case of eclampsia, he remarked: "The essential element, therefore, in the pathology of this case would seem to be reduced to a condition of over-action of the vaso-motor centre, from the stimulating effect upon it of a blood rendered impure through the retention of the collective excrementitious matters which the kidneys ought to have removed. By this over-stimulation there would be produced a high degree of anæmia of the deeper cerebral regions and central portions of the cord, and coincidently a collection of blood in the venous sinuses within the cranium and the spinal cavity, and in the meninges. This result of defective blood-supply would be, at the same time, aided by the peculiar conditions of the venous and blood-vascular systems conditioned by the pregnancy." Dr. Osler very carefully examined, at my request, and found no evidence of this anæmia of the deeper cerebral regions.

Dr. Shakspeare was kind enough to make an inoculation of a rabbit from the medulla and spinal cord, but the result was negative.

*Statistics of Eclampsia at the Philadelphia Hospital.* — In 15 years, from 1874 to 1889, 2,655 women were delivered in the Hospital, and 9 had eclampsia, or 1 in 295. Of those attacked 3 died; the mortality, therefore, being  $33\frac{1}{3}$  per cent. In 2 of the 9 eclampsia began before labor, but in the ninth month of pregnancy. Eclampsia occurred in 2 after labor, in 1 four, and in the other about 24 hours after. Of the remaining 5, 2 were attacked during the first, and 1 in the second stage of

labor; in 2 cases no statement is found as to the time of the attack. The 2 who had eclampsia after labor recovered, the attack being mild; 1 of the 2 in whom the convulsions began before labor died, the other recovered; 2 in whom the convulsions first appeared during labor died. In only 4 of the 9 is there any statement as to albumen being found in the urine, no examination having been recorded as to the others. The patients were all young primiparæ, the oldest being 22 years old.

In the treatment, most reliance seems to have been placed in chloral, morphia, and chloroform inhalation. One patient was bled, but death occurred. In two cases labor was hastened by artificial dilatation, and followed in one by podalic version and delivery, but both patients died.

Let me now add a few words as to the etiology and treatment of puerperal eclampsia. One of the earliest explanations of this disorder I find given by Horn,<sup>1</sup> a teacher and practitioner of obstetrics, in 1765. The disease, he states, is almost entirely restricted to primiparæ because of the rigidity of the parts through which the child must pass, and the powerful abdominal contractions. When the child is forced into the pelvis the uterine nerves are strongly pressed upon, and this pressure is increased by the affluent blood in the arteries; the nerves are so severely compressed that the nervous fluid is driven out of them into adjoining nerves supplying muscles, and hence these muscles are thrown into disorder, and convulsions occur.

Passing from this as one of the curious decayed faiths, we find the way down to the present strewn with once accepted, now rejected, hypotheses. It does not require a long stretch of memory to recall the time when cerebral congestion was the generally admitted explanation of the convulsive seizure; the belief died, but an important part of the therapeutics founded upon it receives more or less acceptance at the present. Blood-poisoning by urea as the cause had its day; that is to say, the convulsions were uræmic. It is no longer an accepted hypothesis, though the term applied to the seizures is still frequently used. Traube, Munk, and Rosenstein held that, in consequence of the hydræmia of pregnant women, when uterine contractions occurred œdema of the brain resulted, which by pressure caused anæmia, and then convulsions and coma followed. Winckel states that he accepted this hypothesis until his experience in the last four years in Munich proved to him that it was erroneous. Many eclamptic women are not markedly hydræmic, and eclampsia has repeatedly occurred in pregnancy, and without the provocation of labor-pains.

Deranged renal secretion, both in character and scantiness, being so

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<sup>1</sup> His work upon obstetrics was published at Stockholm and Leipsic.



common, but not a constant phenomenon of eclampsia, led Halbertsma to attribute the affection of the kidneys to pressure of the gravid uterus upon the ureters. Ludwig had shown that the secretory tension of the kidneys was not great, so that slight pressure upon the ureters suspends the flow. Leyden, in examining the kidneys of women dead from eclampsia, did not find the lesions characteristic of venous stasis, but a simple fatty degeneration of the epithelium. So far the view of Halbertsma was sustained, but the answer will be given in a moment.

Peter, 1865, believed that the poison in the blood producing eclampsia was not a single element, but the various extractive matters which are retained with the urea. The eclamptic is urinæmic. It is because there is an accumulation of all the elements of urine in the blood that she is attacked by the final accidents known under the name of eclampsia. Subsequently, I believe, though I am not positive as to dates, Nothnagel, Strumpell, and Von Jaksck stated that not only urea, but the fixed salts of the urine, retained in the blood caused eclampsia. The urates, etc., of potassium, sodium, and the chloride said in irritating the brain. Fleischer, of Erlangen, believed it was the extractive matter that was contained in the urine which caused the evil; and, further, he was more convinced of this by experiments upon animals.

Winckel, referring to Halbertsma's hypothesis, states that from autopsies made of women dead in pregnancy, and who had not had any indication of eclampsia, he found the ureters had been greatly stretched. He adds: "The conclusion was drawn, no doubt, in many cases from an erroneous hypothesis; to wit, that the renal affection and the scanty secretion were the primary cause, the other symptoms but the consequences, losing sight of the fact that women without albuminuria may have eclampsia, and not considering the possibility that the disease of the kidney may be the consequence of an already present poisoning, just as the convulsions and the coma and as acute yellow atrophy happens with phosphorus, and arsenical poisoning, as proved by Stumpf in two cases in our clinic of eclamptic poisoning." Winckel gives the credit to Battleher of suggesting that the poisoning was from a substance similar to the ptomaines, formed in the living body by decomposition. It should be remembered, too, that Doléris and Butte have found toxic ptomaines in the blood of the eclamptic, while Delore has attributed the disease to microbes. Neither of these hypotheses is so well based upon observations that it can be accepted.

Professor Stumpf's investigations are of great interest. He found invariably acetone in the urine of the eclamptic when the breath very distinctly gave the odor of acetone; and in consideration of the part which, accord-

ing to recent researches, this and similar bodies play in diabetic coma, he looked for sugar in the urine, and found it in all the cases in which a sufficient quantity could be obtained for testing. Hence he came to the conclusion that by an abnormal process of decomposition, a toxic substance, free from nitrogen, — possibly acetone or a similar body with the same reaction, — might by its elimination through the kidneys so irritate as to cause nephritis, effect in the blood destruction of the red globules, greatly change the activity of the liver cells, cause the formation of sugar, the breaking down of the liver-parenchyma to complete yellow atrophy, with the production of tyrosin and leucin, and through the irritation of the brain cause coma and convulsions.

Winckel adds to this exposition of Stumpf's views that the author protests against this theory being applicable to every case, since icterus is not present in all. Nor is his opinion settled as to whether the essential agent is an infection from an external source, arises within the body of the woman, or is derived from the fœtus. This last hypothesis is thought to be not impossible. Winckel states that the greater liability to eclampsia in plural pregnancies, the fatal influence of the disease upon the child, the fact that children with rigor mortis have been delivered from eclamptics by Dohrn and Stumpf, the condition most probably following violent muscular contractions before death, and, finally, the fact to which the author called attention in 1865, that with the death of the child in pregnancy the peril to the gravida is little or none, point to an intimate connection between the mother and the child in regard to the genesis of eclampsia.

It is impossible to give a satisfactory etiology of puerperal eclampsia. Probably the profession is coming more and more to believe that eclampsia results from different causes. Of course, all acknowledge that in some instances it is purely reflex, the irritation being uterine or vesical, for example. But this does not explain the vast majority of cases. I am not sure that the old obstetricians who made the mental condition a factor, the anxiety, fear, sorrow, or other depressing emotion, were altogether wrong; but then such a cause probably can only predispose; so, too, of intense suffering. Passing all these by, there remains the hypothesis of blood poisoning, the poison unknown, possibly not a single one, but several.

Schröder, in explaining the occurrence of eclampsia, has said that it is in the highest degree probable that, as in epilepsy, there occurs, in consequence of contraction of blood-vessels, an acute anæmia of the brain; but the causes of this contraction are unknown. He adds that in pregnancy, in lying-in, and especially in labor, as in children, there is an



increased sensibility of the nerves, so that contraction of vessels occurs from causes which ordinarily would be powerless, and that in labor the vaso-motor centre has a high degree of irritability.

If convulsions occur during labor, few obstetricians accept the maxim of Gooch, take care of the convulsions, and leave the uterus to take care of itself, but endeavor to deliver as soon as possible without violence. A friend informs me that in the obstetric wards of the Vienna General Hospital the treatment is by hot baths, which excite labor-pains and promote perspiration, and cold is applied to the head; during the convulsion chloroform is used by inhalation. For catharsis, senna and colocynth are given, and not salines. Winckel advises in the treatment of pregnant women with notable albuminuria a pill containing one grain and a half of extract of aloes, and the same quantity of extract of colocynth; one to three of such pills are given daily, and the patient also has a warm bath.

In the treatment of the attack he rejects blood letting, as Schröder also did; he directs thirty grains of chloral by the rectum, and inhalation of chloroform. Tittel seems to have had extraordinary success with chloral, only seven of ninety-two eclamptics who were thus treated dying. G. Veit's success with the morphia treatment is also phenomenal, for of more than sixty patients, only two died. Winckel speaks of the doses of morphia given by Veit as colossal, beginning with a hypodermic of half a grain, and increasing the quantity in from four to seven hours to about three grains. But it should be remembered that this treatment was advocated many years ago by our countryman, Dr. Clark, of Oswego, and that his doses were quite as colossal as those employed by Veit. Some doctors reading the results obtained on the one hand by chloral, and on the other by morphia, may hastily conclude that puerperal eclampsia is one of the most manageable of grave disorders. Rude experience may destroy this faith, and they may learn that statistics do not always testify to universal and absolute truth, and that some women perish from eclampsia no matter what treatment is employed.







